TAXONOMY TODAY AND TOMORROW[1]

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There has been a strong tendency toward introspection in our subject in recent years. More than once our leaders have felt impelled to examine the tenets we hold, our methods of inquiry, the fabric of the subject matter, and the theories that motivate our activities. The relationships of taxonomy to other branches of botany have been frequently reviewed. All this "self-inquiry" is healthy. It shows a willingness to extend, modify, or change ideas in conformity with new facts and logical conceptions. However, change for its own sake has no place in this; and those who seek to promote change for its novel aspects should be promptly opposed.

The rapid rise of genetics in the past fifty years has been variously received by taxonomists. Some have shown considerable suspicion of it. Others have gone so far as to give up taxonomy entirely to spend their whole time working in genetics. Actually, though not generally recognized in so many words, genetics has provided taxonomy with a very firm underpinning. From genetics has come an explanation for many things previously well known but unexplained in our field. More than any other branch of biology, it has provided a sound philosophical basis for our activities. The implications of the simple truism that "like begets like" is undoubtedly a very ancient observation of man. Today we can still say "like begets like" if we add "within broad limits" and "under most circumstances." The first qualification permits us to properly interpret "like" to mean

[1] Address of the president, American Society of Plant Taxonomists. Delivered before the Society in Minneapolis, September 11, 1951.

"similar," not "identical." Thus we move from the special creationist idea to a modern viewpoint in which variation is recognized as the normal outcome of a reproducing population of plants. The second qualification permits us to take into account such phenomena as interspecific hybridization, alloploidy, and a variety of special genetical and cytological situations. Furthermore, we may allow for the accumulation of gene-changes and the shifts in gene-patterns that inevitably occur in breeding populations, as well as the introgression of genes from one species into another. Genetics has supplied the basis for these important qualifications. As a corollary to these established principles, it is perfectly evident that the variation that does occur, originating from a given reproducing genetic pattern, is not at random, and there is, therefore, a set of limits to this variation as well as a point at which some expressed phenotypic pattern within these limits reaches its highest frequency.

THE ROLE OF INTRINSIC FACTORS

It should be stressed for our purposes that the most constant and basic characteristics of a given phenotype are the ultimate products of intrinsic factors consisting largely of the genes. Constant and relatively discrete phenotypic characteristics were first used to delineate the Mendelian principles of heredity. Similarly, it is upon these genetically controlled phenotypic characters that a sound classification must be built. Basically, we as taxonomists are interested in the phenotype as an expression of the genotype, not primarily in the genotype *per se* as some have claimed. It need not concern us so much that the relation between gene action and its ultimate phenotypic expression is exceedingly complex. Our attention is of necessity focused on the end-products rather than upon the way in which genes bring these into being. Taxonomic studies are ultimately concerned with whole individuals, groups of individuals, and finally various taxa. Analyses of the multitudinous parts and organs of an individual as to origin, structure, and function are synthesized into an understanding of the whole. Further, the genetic history of individuals and the populations to which they belong provide the basis for considerable knowledge of the relationships of living taxa. Thus they may be viewed from the depth of time. Fi-

nally, we must study the present day and past distributional patterns of species, for in such studies lie the answers to questions as to where they occur on the earth's crust today and where they have occurred in the past. The answers are relevant to an understanding of the species themselves. I am not oblivious to the fact that certain gene-systems are involved in bringing about continuous variation, such as that of size. This type of variation is important in the classification of closely related groups and may be easily taken into account by methods designed to show the total range and greatest frequency of such variation.

The main points I wish to stress are that the major phenotypic characteristics of a living plant are considered to be validated expressions of the genotype, and in using them for taxonomic purposes, we are on the same ground as the geneticists who use them for genetic analyses. We are with good company and the ground would seem to be safe enough. But there are some pitfalls.

In emphasizing the genetic aspects of species differences, some workers have highlighted reproductive incompatibility, even going so far as to make this the sole criterion of species separation. The significance of this type of incompatibility in the evolution of distinct taxa on specific and infraspecific levels is unquestionable in certain groups, but its use as a supercriterion in setting species limits is certainly an untenable procedure. Reproductive incompatibility is not an all-or-none proposition in most instances and it does not arise from a single cause. At one extreme, similar types of incompatibility may exist between different members of the same population, at the other, between different species. Incompatibility is obviously most effective evolutionally when it is associated with other isolating mechanisms. Such isolation is presumably most frequent at the beginning of species differentiation, not an end product of it. The importance of effective reproduction in breeding populations, which in turn make up races and taxa of higher order, is of course fully recognized.

The folly of using reproductive incompatibility as *the* criterion of species distinction has been ably dealt with by Gates (1951). He points out that to insist on infertility (incompatibility) as the sole criterion of species "ignores the aims and methods of

taxonomy and seeks to make taxonomy subservient to a condition—sterility in genetics, by raising it to the importance of a universal principle."

The erroneous idea that species throughout the plant kingdom are essentially equivalents has lead to attempts to discover universal criteria for distinguishing them and to construct definitions that will cover all organisms so grouped. To anyone who has worked extensively in taxonomy, it is obvious that species in different large groups are not equivalents. In most instances they are nowhere near equal. For this reason, a single species definition would seem to be an impossibility no matter how long and involved it was. But if the term species has a different connotation in different groups and it is impossible to define for universal application, then of what use is the word *species* and why do we use it so frequently? These are questions I should like to leave for the moment, to be taken up further on.

THE ROLE OF EXTRINSIC FACTORS

Our intended reliance mainly upon characters whose variation stems from the operation of intrinsic factors makes it necessary to distinguish between these and the characters whose variation is the result of change in the environment. Here lies one of the crucial problems in our subject. For a time, particularly under the leadership of Bonnier (1920) and of Clements (1929), there was a strong environmentalist trend. Environmental factors were held to be of very great importance in molding the ultimate nature of a population and in many cases to outweigh intrinsic genetic factors in maintaining species differences. Today we see the expression of a different form of this notion in Lysenkoism. However, the contentions of Bonnier and of Clements, that one species can be converted into another merely by transplanting it from lowlands to high mountains, or vice versa, has been fully discredited by the brilliant work of Clausen, Keck and Heisey (1940, 1948), who point to the early work of Kerner (1891), with whom they agree. They have demonstrated repeatedly and conclusively that species characteristics, borne of intrinsic factors are relatively stable under radically different environmental conditions. Changes that do occur purely in response to the environment are reversible, giving no indications of perma-

nence. They report that "some of these changes in vegetative characters are quite spectacular, yet they never obscure the individuality of the plant, which is retained irrespective of the conditions of altitude, light, and moisture in which the plant is grown." I am sure you are all familiar with these studies and the conclusions derived from them.

Assuming that all of us will not have available the extensive garden facilities that are needed to determine experimentally the nature of the variation in given populations, we are often required to attack our problems in a different way. When this is necessary, we may fall short of the ideal. However, in all instances we should carry our studies as far as facilities and materials will permit. Students, particularly, should be encouraged to utilize every means at their disposal to make the results of their studies as nearly perfect as possible.

It has long been known that the overall dimensions of the plant-body and its vegetative parts are sensitive to external influences. Because of this, we have the axiom in plant taxonomy that qualitative differences in the vegetative body are more significant for classificatory purposes than are quantitative differences. Furthermore, structures maturing decisively at an early stage in the formation of vegetative parts are less subject to environmental influences than those with a prolonged formative period. For example, most trichomes when present on the leaves of Angiosperms, mature and the cells lose their living contents at a very early stage in leaf development. For this reason, they are far less apt to vary quantitatively due to extrinsic factors than the leaves themselves.

Though dealing with the whole plant, we select for special attention features reflecting most accurately its hereditary constitution. The reproductive parts and associated structures are in general less sensitive to environmental influences than the vegetative portions of the plant (Anderson, 1929; Turrill, 1936). Here, relatively large quantitative differences are more likely to be significant than in the vegetative structures. But again, greatest emphasis for taxonomic purposes is placed upon the qualitative aspects of the structures involved. Polyploidy and the frequently associated size-effects in many plants are too well known to be ignored.

The Role of the Herbarium

We have heard a good deal in taxonomy recently about the study of dynamic populations, of natural hybridization, of introgression, of gene ecology, of apomixis. These modern aspects of some of our problems are popular, and studies involving them carry an implication of *right*. From them we stand to gain a more penetrating understanding of various plant groups and their biological nature. Unfortunately, accompanying increased activities along these lines one finds a tendency to deprecate "specimens," and hence the herbarium and, for that matter, classical taxonomy comes in for its share of punishment. For a moment, I should like to say a little about the role of the herbarium in taxonomy. Actually, we have in it a unique method of documentation. Specimens so placed are relatively permanent and are scarcely subject to manipulation to obscure the facts they reveal. In them, investigators for centuries have available, as nearly as one could hope for, exactly the same basic materials. The specimens should be looked upon as samples. As such, they may be studied in a wide variety of ways. They are samples of the taxonomic groups to which they belong; of a particular flora; of a plant association; of a specific population; of a particular combination of morphological and anatomical characters; and of the product of a particular set of genes. Whether any set of specimens fully represents a given taxon obviously depends upon the circumstances. It is doubtful whether any herbarium has specimens showing the complete variations of more than a few species, if even one. However, one good specimen is concrete evidence that a particular taxon exists or has existed, and considerable information can be obtained from it. It is self evident that the amount of information obtainable does not double when a second specimen of the same taxon is acquired. The amount from each new specimen of a representative series will never equal that obtainable from the first. For this reason, the extent and kind of specimen-representation in a particular herbarium will depend largely upon its objectives and facilities. Perhaps it would be ideal to have the total variation of all taxa represented by specimens in some herbarium, but such a goal is impossible of achievement. Merely to determine the total range of morphological variation of a given taxon, using all the means and meth-

ods available, is practically an impossibility, let alone having it represented in an herbarium.   Assuming the latter were possible, continuous evolutionary changes would often make such a representation obsolete.   This type of natural limitation is not confined to the taxonomic side of things but is equally operative in other branches of botany.   The problem of adequate samples is ever present.   However, considerable communicable knowledge concerning various taxa can be assembled and a lot can be represented by specimens in the herbarium.   It is to the possible that we direct our attention.

The system of arranging specimens in an herbarium is impressive.   The availability of any one of the one million three hundred thousand specimens in the Gray Herbarium never ceases to be a marvel to me.   Our specimens are far more accessible than the books in a large library.   The key to the arrangement is the system of nomenclature which of necessity places the specimens under species names.   Our esteemed friend and former president, Dr. Camp (1951), seems to feel that such a procedure leads many taxonomists to regard the *specimens* so placed as the *species*. Perhaps such a tendency exists, but it is hard to imagine a more satisfactory way of doing the job.   Personally, I have never met a taxonomist who would argue that a species resided in a museum case.   Representatives or samples of a species, yes, but not the species.   When I climb to the twelve-thousand-foot level on Hoosier Ridge in Colorado and see individuals of a certain mustard growing among the rocks, I may pick up a plant of it and remark that it is *Draba crassa*.   But I do not delude myself into thinking that that particular plant is the species, for I know there are thousands of individuals on Hoosier Ridge, not to mention the tens of thousands on the other high peaks of the Colorado Rockies.   Rather, the plant I held in my hand was a sample of *Draba crassa*.   One might, in like manner, contemplate a particularly vigorous Jack Pine in northern New England, and suddenly exclaim "Why this is *Pinus Banksiana*."   He would not thereby declare that particular tree to be *the species*, for he would know there are hundreds of thousands of living trees of Jack Pine to be found anywhere from Quebec to Saskatchewan. We work by sampling in the field as surely as we do in the herbarium, and both have the natural limitations inherent in the

technique. I doubt whether anyone is capable of a full compre-
hension of the diversity represented in all the individuals of a
single populous species.

THE ROLE OF EXPERIMENTS[2]

There is a popular notion that the real and the only way to get
at the true nature of a given taxon is to go at it experimentally.
Bring the plants or their offspring into the experimental garden
where they can be manipulated in accordance with our best-
planned experimental procedures. The results, when properly
codified, tell us considerable, but for taxonomic purposes it is
often in the nature of additive information to that obtained in
other ways, and rarely replaces or negates it. Personally, I
believe the more experimentation designed to reveal the nature
of species and other taxa the better, but experimental taxonomy
is not the whole answer to the problems in our subject. Experi-
mentation is beset with limitations, as are all methods of investi-
gation, and this we must recognize. In the first place, experi-
ments must be carried out by using samples and they are, there-
fore, subject to the natural limitations of sampling techniques.
Secondly, it is next to impossible to sample most taxa adequately.
In my own experience, forty acres of experimental plantings were
wholly inadequate to provide a proper overall sample of *Par-
thenium argentatum*, a species comparatively restricted in its
geographic range. How many of us have forty acres available
for expermental purposes?

It seems almost redundant to say that critical observation and
study in the herbarium alone, in the field alone, in the experi-
mental plots alone, or in the laboratory alone, are insufficient for
the realization of the primary objectives of taxonomy. A com-
bined attack, using to the fullest the techniques of all these, will
scarcely be sufficient to complete the job, but we should get
closer to our goal using all of them than we would by using but
one. If, in addition, we borrow as much information from other
branches of botany as is pertinent and possible, we should proceed
a step further toward our ultimate goal. Thus viewed, taxonomy
becomes an integrative and synthesizing subject, in a way rising
on the shoulders of its sister disciplines.

[2] It is not my purpose to develop this subject in the manner that might be implied
from the present heading for, to do so, would occupy my entire time.

What Is a Species?

One of the stumbling blocks upon which the introspective wanderer inevitably barks his shins is the precise definition of terms. Whether we like it or not, the precise meanings of many scientific terms begins to be altered immediately after they have been proposed. After a few generations have been at work, their meaning has often been altered completely. As ideas and ways of looking at subject matter change, the meanings of applicable words are shaded to accommodate the new view. Shifts in meaning are usually so gradual and so closely tied in with the current of contemporary communication, that they frequently go unrecognized until considerable difference from the original has developed. The words we have the most trouble with are those closely associated with changing concepts. For example, to the pre-evolutionists the term species was definite enough. The immutability of its meaning was immediately upset by the concepts of evolution and its preciseness was further devalued by the impact of genetics. Now we are told that no one knows what a species is, and that perhaps there isn't any such thing anyway. Yet anyone who has dealt with whole organisms, whether plant or animal, complex or simple, is fully aware of certain patterns of populations that are reproduced with fidelity over great periods of time. He knows with certainty that at a given time level, the members of neither the plant nor the animal kingdom represent a continuum, no matter what the vantage point from which they may be viewed. Given the facts that organismal diversity exists and that it does not exist in a continuum, the taxonomist's job is to assemble and systematize a body of communicable knowledge about the different kinds of the earth's plants, both of the present and of the past, that will reveal their true nature. In this, the problem of species remains a major one in spite of all that has been said or written upon it. I dare not venture a guess as to the number of times the species problem has been discussed. Certainly it is a perennial of many years standing and it appears to grow more lustily as the years go by. It seems generally agreed among most discussants and writers that a universally applicable species definition is a must and that without it taxonomy lacks the dignity of a science. Some have viewed the problem of defining the term species as a

futile one, others lifted the problem to a light vein by suggesting that species are completely subjective and are what a good taxonomist says they are. In taxonomic literature, we even see descriptions of new concepts instead of new species.

Since *species* is one of the key words in our subject, we shall continue to be badgered by questions for an explanation of it. There has often been a certain amount of irrationality in the way of coming to complete grips with the problem. We have sought to satisfy the philosopher-logician type of inquiry by trying to provide a definition of the term. Naturally, others would like to know what we mean when we speak of a species, whether it be of an alga, a conifer, or a mint. But I think none of us have really believed that the curious could find out on his own what a species was in any one of these groups by using our numerous and varied definitions. Even the incompatbility test, which perhaps came as close to being practical as any, brought forth only negative evidence in many instances. If two particular plants of presumably different species would not cross and produce fertile offspring, one could never be sure that two other plants representing different strains of the same two presumed species would behave similarly. In specific groups where authorities supposedly held the secret as to what the species were, there were always intangibles and indefinable somethings that aided in species determination. I do not minimize the necessity of knowing a group nor of diligent attention to every possible detail. The point is, the species definitions we have concocted are not practical. I happen not to agree with most of the attempted dispositions of the problem of determining what species are, and, for this reason, feel somewhat justified in speaking about it. Certainly the problem is very much with us and still needs attention.

In the first place, I should like to state my unqualified belief that there are species in nature quite apart from man's contemplation of them. It is not necessary to call them species, but there are groups of closely interrelated organisms that reproduce themselves with a surprising fidelity over long periods of time— and I am thinking now in terms of millions of years. These species are not equivalents in different large plant groups and, considering the tremendous diversity in the plant kingdom as a

whole, it would be surprising if they were. The species in relatively unrelated groups do not have the same attributes, they did not arise in the same way, and they most probably have been subjected to a wholly different combination of environmental influences during their phylogenetic life. Why, then, should we be surprised that a universal species definition for them seems impossible? I do not agree with the notion that because we do not have a satisfactory man-made species definition, the whole fabric of taxonomy comes crashing down around our ears. The species are not man-made. The difficulty arises from trying to pour into a single kind of mold the thousands of almost totally different kinds of plants all will agree are represented in the plant kingdom. The problem is one of interpretation. But why try to produce a universal definition? Why not let the species themselves tell the story? In order to elucidate this latter question I must start with some basic facts.

Genetics has provided us with a valid principle upon which to build. This may be stated as follows. The closer the genetic relationship of individuals or groups of individuals, the greater the number of genes they have in common. Conversely, the more distantly related the fewer common genes. Remembering the specificity of gene-action, it is obvous that similar or identical genes and gene systems produce similar end products. Thus we are justified in using the phenotype as an indicator of relationship. If, as we must assume, similar genes provoke similar organizational patterns, then it follows that genetic variation is controlled in a like manner. Indeed, this has been shown many times. Here is the crucial point, providing the basis for taxonomic use of the organizational pattern of an individual as well as the variation of such a pattern that may be found among all the individuals making up a species. It is in the area of variation that we have learned most in recent years. From the main principle springs the logical basis for the comparative method.

How distant may the relationship be of two groups of plants and there still remain common gene lineages that will operate to produce detectable specific similarities in the phenotype? This we do not know, and there is no organized information on the point. However, parallel variations and changes have been repeatedly observed in species known to be related. It is no

stretch at all to suppose that the species of any given genus, or at least subgenus, have numerous common genes and, therefore, we can expect similar characteristics, similar patterns of variation, similar modes of genetic change and even similar responses to external factors. That such is actually the case in many genera can readily be shown. Similarities of this sort permit us to extrapolate from one species to related species. Between certain closely related genera there must also be a community of genes and gene patterns. The species most difficult to distinguish, using all types of criteria, are the most closely related. These have been the source of greatest consternation because of the difficulty of knowing where to draw the line between such species and infraspecific taxa. "Where does the species stop and the sub-species or variety begin?" is the question frequently asked. How many different genes must two populations of plants possess before they are species? How many similar genes may they possess and remain on the species level? These are pertinent questions, but they cannot be answered for the vast majority of cases in which they may be asked. Our present knowledge of most plants is much too fragmentary to provide exact answers. But if we should ask how many and what type of different phenotypic expressions of genes are required before a group of like plants are a species, or only part of a species, there is a way of providing the answer. This way is based upon the expected similarities of related species as mentioned previously and the use of comparative procedures.

The Species-standard Method

For the present, let us focus our attention upon the so called difficult genus *Arabis* of the Cruciferae. It is not an extremely large genus, having roughly 65 species in North America and a few less in the rest of the world. Nor is it small as genera of Angiosperms go. Within it are complexes of species difficult to interpret and separate. Polyploidy is present and interspecific hybridization occurs. At least one species is apomictic (Böcher, 1951), and the rest have not been studied from this point of view. But there are also perfectly distinct, well-characterized species in the genus, too. Often they have distinctive geographic ranges. These distinct species are obviously so, to any discerning taxon-

omist, and one could obtain complete agreement among botanists willing to study them more than superficially. When I worked on *Arabis* as it occurs in western North America some years ago (Rollins, 1941), I studied these well-defined species in as much detail as possible at that time. They provided the basis for interpreting the more difficult series. They were biological standards of comparison, so to speak, that revealed in themselves what a species is in *Arabis*. There was no need for arbitrary species criteria nor man-made definitions. To have used them would have obscured the facts as revealed by these clearly defined species. In this study, the species used as standards were *Arabis blepharophylla*, *A. cobrensis*, *A. Crandallii*, *A. crucisetosa*, *A. Cusickii*, *A. dispar*, *A. furcata*, *A. glaucovalvula*, *A. Koehleri*, *A. Parishii*, *A. perennans*, *A. platysperma*, *A. rectissima*, *A. Schockleyi* and *A. suffrutescens*. Among them are representatives of all four groups or series of species of *Arabis* occurring in the area. Some of the species are widespread, others are restricted geographically. Some showed less variation than others. In all cases the patterns of variation were of the same general type. Similarly, the same pattern of definitive characteristics ran through the entire group. While the species-standards, individually and collectively, were the real key to species interpretation in the whole genus, certain working hypotheses as to the value of particular characteristics emerged from a study of them. For example, the presence or absence of trichomes on the leaves and stems of *Arabis* was found to be an unreliable criterion of species difference. On the other hand, qualitative differences in the trichomes proved to be of considerable value.

The method used to determine "what is a species?" in *Arabis* is one partially used by many monographers, though in most instances it is not done in an organized way. It is my conviction that an extension of the method is highly desirable and would go a long way toward solving the practical aspects of the "species problem." The use of biological standards of comparison is an accepted procedure in other branches of botany. The *Avena* coleoptile test, the *Stapholococcus aureus* test for penicillin concentration, and the growth-rate test of chemical concentration in *Neurospora*, to mention only a few, all depend upon a completely biological standard. Many biological stains and reagents are

standardized by purely biological tests. In numerous instances, whole organisms are involved whose complete development is no more controlled nor in fact known than are the wild plants with which we work. The use of living and dead plant samples in formulating species-standards and the use of these in determining the species of a given group, is vastly more complicated than the tests mentioned above. But the method is sufficiently extensible to meet our requirements.

What are the possibilities and ramifications of the species-standard method? Perhaps this can best be shown by assuming that we are setting about to determine "what is a species?" in a hypothetical genus. First we study our materials sufficiently to be able to select for intensive investigations those species that are readily discernible. These materials may be specimens in the herbarium, populations of plants in the field, or plots of them in the experimental garden. In a well-kept herbarium having a reasonable representation of the genus, this step will have been largely done. In many herbaria, the specimens will reflect the cumulative wisdom of a number of workers who have studied them. Study of the geographical ranges of distribution often aids materially in this first stage. I anticipate that someone will say at this point that there are some genera in which there are no clear-cut, easily recognized species. Personally, I do not know of a single genus in which such species may not be found. Certainly the notoriously "difficult" genera *Poa*, *Salix*, *Rubus*, *Crataegus*, and *Hieracium* have readily distinguishable species that might be utilized for our purposes. Naturally, it is necessary to exercise some judgement in the selection of these species and the competence of botanists will vary in this regard. But the critical aspects of the selection are so unrefined as to be within the scope of all who are trained. Furthermore, botanists outside of taxonomy with any acquaintance with its aims and methods will find the selection easy to follow.

The second step in our inquiry involves the detailed study of the selected species. Ideally, this should extend all the way from a careful analysis of the characteristics of a representative series of specimens to transplant work in the experimental garden. Data concerning the range of variation of the species in the field and under manipulated or selected environmental

conditions should be gathered and processed. The morphology, anatomy, and cytology should be worked out with a view toward deriving useable information for taxonomic purposes. The reproductive cycle and mode of pollination should be studied. In short, these selected species should be subjected to the most thorough-going study possible. That is the ideal. Actually, most taxonomic studies will fall short of this ideal, except in a limited number of cases. But until such studies are made throughout the plant kingdom, our work is not finished. From this point of view, it is evident that we have scarcely started on the job ahead. The successful application of the method is not dependent upon any particular level of refinement or thoroughness of study of either the species-standards or the other species of a genus under consideration. The most accurate and reliable results will come from complete studies using wholly adequate materials. Where only the minimum of specimens is available and there is no opportunity to carry out experimental or field studies, the method can still be used with considerable probability of arriving at the proper species designations.

The method I have briefly outlined to determine "what is a species?" has several aspects that tend to clear the path of the logical impasse often cited in considering the species question. (1) It is free from arbitrarily selected criteria. Under this scheme, incompatibility or any other characteristic may be significant or insignificant, depending upon the group involved. (2) A universal definition is not required. The species are probably of a different sort in every major group. By recognizing this, we recognize the very great organic diversity existing in nature. Furthermore, we recognize the numerous ways in which species arise and the variety of circumstances under which they survive. (3) Species so defined are not a subjective creation nor a mere concept of man. However, a high percentage of agreement of interpretation should be possible. It should be clear that the species exist, independent of man's ability to define them or to perceive their presence. The fact that many species have reproduced themselves with fidelity for millions of years is ample proof of this (Stebbins, 1950). (4) The method is logical and largely objective, depending not on judgement alone but utilizing the best procedures in scientific research. Naturally,

some judgement is required. No matter what is often claimed,
there is a certain amount of subjectivity in all kinds of scientific
inquiry. If this were not the case, neither ours nor other sciences
would have any real meaning to man. (5) The method is ex-
tremely flexible, providing the procedure for determining the
species in all groups, in such a way that it can be followed by
anyone with sufficient training and interest. (6) It permits the
assembly of an almost unlimited body of knowledge about
determinable and redeterminable kinds of plants. This knowl-
edge thus becomes communicable and the basis for its organiza-
tion on a large scale is provided. This I consider to be one of
the major aims of taxonomy.

How critical is the species-standard method? Simply an-
swered, one might say that the method is as critical as the time
and energy of the botanist making the study will permit. The
implication is that there are unlimited possibilities for the study
of the species providing the standards of comparison within a
given genus. This is equally true of the more difficult species
that are dependent upon the use of these standards for their
detection. The accuracy of the comparative procedures used for
the latter purpose is dependent upon the precision and extent of
the various comparisons made. Certainly, accuracy cannot be
achieved without great care. Complexity stems from the com-
plex nature of the organism itself and perhaps can never be
avoided in taxonomy. As stated before, we deal in whole
organisms, but the studies employed ordinarily involve analytical
procedures in which any part of the plant may be the subject of
attack. Such analyses may range from studies of chromosome
behavior to the gross morphology of the flower. In the end, the
results of analytical studies are synthesized into a body of knowl-
edge that forms the basis for making appropriate comparisons.

As a general observation, it seems to me that taxonomy fails
to appeal to some scientists because it does concern itself pri-
marily with whole organisms, which are by their very nature
exceedingly complex. Chemistry, Physics, and Astronomy have
influenced all of science in the direction of desiring a one to one
relationship between cause and effect. The nicety of mathe-
matical precision has been a constant goal in the reduction of
observation and experiment. More recently, these sciences have

dealt with variable systems and the very complex relationships of various phenomena have been widely recognized. In spite of this reversal, the trend in many branches of biology has continued in the old path. On the whole, the struggle of biologists to simplify the interpretations of phenomena of living organisms has not met with spectacular results, but the directional emphasis has remained. As taxonomists, we find ourselves well outside the main stream of this activity because of the nature of our subject. Simplification cannot logically be carried below the complexity inherent in the living individual and the population of which it is a part. I do not say that analytical methods of all sorts may not be used to delve below this level, for it is extremely important that we utilize them to the utmost, but the information thus obtained, to be relevant, must be applicable to the plants as a whole. We are thus dependent upon information obtained largely by analytical methods but in its ultimate, usable form, it has to be synthesized into a whole. We are dealing largely with variables and the complexity of the ultimate systems represented by the living individual, the population, and the taxon considered in time is so much greater than known physical systems that there is no real basis for comparisons between them.

What would be the consequences of the widespread use of the method I have outlined? If the taxonomists who have described hundreds of apomicts in *Hieracium* had used it, certainly they could not logically have named them as species. It would have been absurd for me to have named as species fifty to a hundred apomicts in *Parthenium*, and I am convinced that the wholesale naming of apomicts as species, wherever they are found, is equally absurd. Babcock and Stebbins (1938) using the sexual species of *Crepis* as reference points for the organization of the apomicts in that genus, were certainly on the right track. I believe that the method would even permit agreement as to the numbers of species of *Rubus* or *Crataegus* in eastern North America. Perhaps I am unduly optimistic here, but I should like to see it given a trial.

Using the species-standard method, it would be impossible to recognize socalled cryptic species which show no morphological singularity even though they were incompatible with morphologically similar plants. But I do not believe these are, in reality,

species. Difference in chromosome number alone would scarcely be sufficient to set two species apart unless that were the rule throughout the genus. It would not help particularly in dealing taxonomically with species-hybrids and cases of introgression. However, many instances otherwise not easily seen might be detected. It is impossible to foresee the total ramifications of the wide adoption of such a procedure. It does throw emphasis toward the monographic type of study rather than the floristic, for determining what the species are to begin with. Such a procedure has long been recognized as desirable. The taxonomist, working with a particular flora without adequate reference to monographic studies, is indeed handicapped in arriving at natural specific boundaries. The danger of misinterpretations, when a single species or only a few species of a genus are studied independently, should be immediately apparent.

The two novel aspects of the proposed method are (1) the complete abandonment of any attempted species definition that would be of universal application, (2) the definition of species at a relatively low hierarchial level in the plant kingdom, i. e., probably most frequently within the genus. Thus a paradox of long-standing would be resolved. These are fairly radical deviations from the most common present day ideas, so that one may ask how these changes would affect traditional taxonomy as well as the newer approaches to the subject.

In the practice of traditional taxonomy these changes should result in considerable improvement. Definite reference points within genera could be set up that would have real meaning to a wider audience of botanists. Greater agreement in interpreting difficult genera should be achieved and a more systematic determination of the species in generic revisions should result. Nomenclature does not enter, except as new interpretations call for nomenclatural adjustments. Insofar as the newer approaches to taxonomy are concerned, such as those developing in cytogenetics, experimental taxonomy, and the like, these would be as relevant as before and perhaps more so, for the information obtained could be brought more directly to bear on the practical problem of species interpretation. Thus, an integration of the old and the new in taxonomy might be envisaged for the ultimate benefit of us all.

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THE GENUS TOVARA (*POLYGONACEAE*)

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THE broad concept of the genus *Polygonum* has been subject to repeated splitting in the past. In most cases, these segregates, maintained by certain authors but generally not accepted by most other taxonomists, have often been based on vegetative characters which are found to vary greatly in this group of plants. *Tovara*, however, is very distinct in having constant differences in the inflorescence and floral parts. Currently it is recognized by most authors as a generic entity distinct from *Polygonum*. The differences are as follows: In *Polygonum*, the calyx is 4–6-, mostly 5-parted, enlarging in fruit, and the segments are often

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